

## “QUIT-CLAIM” SPECIALISTS VS. THE MAKING OF MANUALS.

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“The ground work, the foundation of the whole immense entomological structure of to-day is essentially taxonomic. . . . Objects without names cannot well be talked of or written about; without descriptions they cannot be identified and such knowledge as may have accumulated regarding them is sealed. . . . In short, without the fundamental work of the taxonomist, the great mine of entomological literature would not exist; the accumulation of knowledge would be largely limited to what each person could personally observe and remember.”<sup>1</sup>

“Entomologists clearly recognize the existence of a shortage of systematists and the necessity for supplying the demand. Can those having the power to meet this need be made to see the present difficulties of the situation and provide a living wage for each of those human beings who are willing and desirous of devoting themselves to insect taxonomy?”<sup>2</sup>

“Taxonomy demands the highest talent, and those who prove their fitness should have every facility and inducement.”<sup>3</sup>

With the above quotations as a justification for the preparation of my manuals, I wish to put on record some of my experiences during the past twenty years—some of the obstacles with which I have had to contend in the work that I set out to do, and that is now practically completed. I also wish, for the first time, to answer as far as I can, some of the criticisms regarding that work. In so doing I fully realize that my critics will probably become more embittered and may exhaust their vials of gall in reply, but I long ago learned that: “Truth wears no mask; bows at no human shrine; seeks neither place nor applause; she only asks a hearing.”

In 1907, realizing the great need of general works, descriptive of certain groups of insects with which I was somewhat familiar, I began the preparation for the novice or beginner of those manuals which have been put forth in the twenty years which have

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<sup>1</sup> Gahan, A. B., *Proc. Entom. Soc. Washington*, XXV, 1923, 73.

<sup>2</sup> Editorial in *Entomological News*, XXXVII, 1926, 117.

<sup>3</sup> Aldrich, J. M., *Science*, April 22, 1927.

elapsed. In their preparation I have given free, and solely for the good of the cause, not only all my spare time but more than \$12,000 of my previous earnings for their illustrating and publication. This does not include the salary of my faithful assistant, who by thirty years' training and practice, has been able to put my long-hand hieroglyphics of key and text into typewritten manuscript which the printer could use. The demand for such works as I have put forth is limited, and for that reason I am as yet more than \$4,000 "to the bad" in their publication.

In the preparation of the first two works issued, the "Coleoptera of Indiana" (1910), and (in collaboration with Chas. W. Leng) the "Rhynchophora of N. E. America" (1916), I succeeded, without much trouble or delay,<sup>4</sup> in getting such aid as I requested, and when completed there was little published criticism of these works.

In those days there were fewer specialists than now who were imbued with the idea that the Good Lord had given them a "quit-claim deed" to all the species in this country belonging to their especial group, and that no one, not even the author of a manual who desired to include *all* species from the territory he was covering, had any right to "poach on their preserves." When, however, I began work on the Orthoptera of N. E. America I found that these specialists had "arrived." My experience was exactly that set forth in a recent article,<sup>5</sup> *viz.*, "Suppose some specialist in a museum has published many descriptions (say 500) of species in his particular group. The specialist, being a mere human, may begin to get chesty about his control of the group and, entrenched behind his 500 descriptions, may boast, 'Anyone that wants to study the Utopiidae has to come here; I've got the group so tied up they can't wiggle.'" I was unable to borrow along as I needed them a single specimen from the specialist I have in mind. Many excuses for not making the loan were given and, in addition, the following free advice was offered: "Personally I would be very chary about either preparing or attempting to publish a work on eastern U. S. Orthoptera at this time for the following reasons: (a) The Melanopli must be completely studied from new angles and in a number of other groups the present order or arrangement, and even recognition of forms,

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<sup>4</sup> See p. 5 of the Coleoptera and footnote.

<sup>5</sup> Proc. Ent. Soc. Washington, XXVII, 1925, 185.

must be greatly modified. (b) The ultimate object of the field work my co-worker and myself have been doing for the last sixteen years is the preparation and publication of a work on the Orthoptera of North America which will be on the scale of Scudder's butterfly work, and which we hope to have ready in ten or fifteen years. (c) It would be almost impossible to get such a work published now by an institution or society, as retrenchment must be in order for the period of the war."

When this letter was received I asked myself the question—"How is the student or beginner going to name his specimens during the ten or fifteen years which must elapse before this gigantic new work by this Orthopterological combine is ready?" As there was no satisfactory answer I went ahead and prepared my manual and published it myself and am glad I did so, as ten years have since elapsed and no further notice of the intended publication of the big work of the firm has since been given.

What was the immediate result of the publication of my manual, contrary to the advice of this specialist? A four-page diatribe in *Entomological News*, in which there was scarcely a single sentence of approval of my work. Aside from this diatribe, the "Orthoptera," and the two preceding works, were well received by those who really needed them—*viz.*, the tyro or beginning student and the busy economic entomologist who wished some one work by which he could quickly and readily identify his specimens in hand.

When I began work on my last manual—The Heteroptera of N. E. America—I soon found out that the "quit-claim" specialists had increased in number. To one of them who is connected with a prominent museum and who, with a co-worker, had just finished some Revisions or Monographs of special groups of Heteroptera, I sent some Florida specimens for naming which belonged to those groups, but which I could not identify from his recently issued monograph, asking him to return them long enough for me to draw up descriptions from them. He returned them, three of them marked "unknown to me" or "new species." At the same time he wrote: "I think that harm is done the study every time that descriptions are published apart from present or recent synoptical work. I trust you will not send me any more under the same conditions." I, of course, did not send him others, but I described the new species, as I had to have a name for them in order to place them in my book.

This same specialist afterward published an article in which he favored the loan of type specimens from museums. As I had had trouble in borrowing even paratypes from his museum, I wrote and asked him why he could not "practice what he preached," and lend me a few specimens to help me in my work. He replied: "My paper contained my personal views, but the rules of the museum forbid the loan of types. However, I do not consider the preparation of a manual as falling within the class of work to aid which I would urge the loaning of types. Only thorough-going revisional work deserves such signal assistance."

Another one of these "quit-claim specialists, and probably the king-pin of them all, is Dr. H. H. Knight, of Ames, Iowa. He recently published<sup>6</sup> a caustic eight-page criticism of the "Heteroptera," especially that portion of it pertaining to the family Miridae. About the confines of this family, as represented not only in this country but in the world at large, he has attempted to construct a hog-tight, bull-proof fence and woe be unto any person who attempts to root his way under. The only line of commendation (?) of my work in Knight's article is as follows: "This book *exhibits unusual ability in the compilation of the subject-matter* [sic], chiefly from the 'Hemiptera of Connecticut,' but the author has also *mixed in much* new material. In so doing he has fallen into several errors."

Now the "Hemiptera of Connecticut" should be rightfully entitled the "Hemiptera of New England," for of the 750 Heteroptera treated in that book, only 354 were recorded from Connecticut. In that work 255 genera of Heteroptera were treated, the majority of them only briefly in keys, without any generic characterization whatever. In the Heteroptera of Eastern North America, 398 genera were treated, not only in keys but with a full characterization of each genus. In the Connecticut work 750 species of Heteroptera were treated, many also often only briefly in keys. In my work 1,253 species were treated not only in keys but with a full description of each. The words "mixed in much new matter" are, therefore, probably justifiable.

Knight states that three of my new species of Miridae are synonyms and that 12 of those listed do not occur in the territory covered. I wish to take up briefly each of these in the order mentioned by him and show why the error, if any, occurred.

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<sup>6</sup> Bull. Brook. Ent. Soc., April, 1927, 98.

*Eioneus gutticornis* sp. nov. According to Knight this is *Dolichomiris linearis* Reuter. He is probably right, but Reuter's species was described from West Africa, and until I took it in Florida, had not been taken within 1,500 miles of this country. Is it any great wonder that the error was made?

*Mimoceps gracilis* Uhler. Knight's article (Can. Ent., 1927, 41) stating that *M. gracilis* is only a color variety of *M. insignis*, did not appear until six months after my book. I am not a seer, and as I was unable to borrow specimens of *insignis*, I published *M. gracilis* as it was then of record.

*Platytylellus confraternus* (Uhler). Here again Knight's paper in which he "inclines to believe" that *confraternus* does not occur in the eastern states did not appear until after my book was issued. The insect had been recorded from New England by Uhler and Parshley, and there was no evidence that their records were wrong. Knight, without seeing the specimens on which these records are based, states that they "probably refer" to his *fraternus*. Moreover, when I first sent to Knight the Dunedin specimen on which he based his var. *collaris*, he wrote: "I am holding for study a female of *Platytylellus* which is certainly very close to *P. confraternus* Uhl. from Colorado. I need a male specimen to decide with certainty." However, he went ahead and described the variety from my Dunedin female and one other female from Gainesville, Fla. Now, in my opinion, Knight to the contrary notwithstanding, the var. *collaris* is only a color form of *confraternus*. I included and described it as such and *P. confraternus* is, therefore, represented in Florida, as stated by me.

*Paracalocoris incisus* Walker. On October 24, 1921, five years lacking five days before my book appeared, I sent Knight, at his request, certain species of the genera *Platytylellus*, *Pilophorus*, etc., for study and report. Under date of July 21, 1923, one and a half years later and three and a half years before my book appeared, he made a partial report on the more common species I had sent him. In this letter he wrote: "Among the specimens you sent as *Platytylellus* there were three of a species of *Paracalocoris*, and these I am holding for further study." He did not write, as he states, that he was "publishing," but continued to hold them without giving me any name whatever. In July, 1926, two years later, desiring to include the name and description of the bug in my work, I described it in Ent. News as *P. novellus* sp. nov. Later Mr. W. E. China, of the British Museum, to whom

I sent specimens of the *P. novellus* as named, informed me that it was the same as *P. incisus* Walker, and I included it under that name in the Heteroptera.

Mr. China also wrote, "The *Capsus externus* H.-S. of Walker (1873, 91) is possibly another variety of *P. incisus*. Whether they are distinct species or merely color varieties of the same species I am not prepared to say without examining a series of specimens." Knight, without seeing the type of Herrich-Schaeffer, and judging solely from the colored plate of that author, has made *P. incisus* Walker a synonym of *externus*. He has also, in a paper issued December 20, 1926, described and named four "spotted-dog"<sup>7</sup> color varieties of *P. externus*. All of these varieties can occasionally be found on one plant, where they are evidently the progeny of one mother. They differ only in the relative amount of red and fuscous on the pronotum and scutellum, yet a scientific name is given each of them by Knight.

*Phytocoris megalopsis* sp. nov. If, as Knight states, this species is a synonym of his *angustifrons*, it is due to his failure to return to me for examination until too late my specimen upon which he based his description of *angustifrons*. I was unable to get back my examples of any *undescribed* species of Miridae sent him, or even the generic name of any of them, until after the greater part of my Miridae paper was in type. On April 29, 1926, I finally wrote and *demand*ed that he return them so that I could draw up descriptions from those represented by uniques and include them in my work. On May 7 he returned some of them with his names attached, and stated that he would send the others when I returned those. On May 19 he sent the remainder, including his holotype specimen of *angustifrons*, which I had taken at Dune-din, Fla. Meanwhile I had described what proved to be another species as *angustifrons*, and it was then too late to make the change.

*Phytocoris rubellus* Knight MS. Under this name Knight states that in his paper, issued October 6, 1926, "Seven species are described from the eastern United States that are not included in Blatchley's book." Of course they were not included. His

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<sup>7</sup> By "spotted dog varieties" I mean those which vary in non-constant color characters, as do the pups in a litter, only in the relative amount of dark and light colors. See page 8 of the Heteroptera.

paper was not published until my book was being bound. He had refused to lend me any specimens whatever and I was lucky to get back his unique types of my specimens of *P. albitylus* and *P. rubellus* long enough to draw up descriptions and affix his names to them.

*Creontiades flicornis* Walker. In a footnote to page 884 I mention that China had informed me that the *Eustictus grossus* of Uhler is a synonym of this species. This information came too late to change the status of *flicornis* and *grossus* in the text. If Knight thinks that *flicornis* is "such a distinct form that there could scarcely be any mistake in placing it in the keys," why did he not so place it in his "Monograph of Deræocoris" and in the Hemiptera of Connecticut?

*Labops hesperius* Uhler. My opinion on the relationship of this form and *hirtus* still stands as expressed in my book. Examples of both eastern and western forms were at hand when my description was written.

*Pilophorus brimleyi* Blatch. This species was described as a *Pilophorus* before I had opportunity to study the generic description of *Barberrella* Poppius, his paper not being available to me until July 19, 1926. Knight, in the Hemiptera of Connecticut, where his *B. apicalis* was described, gave no characterization whatever of the genus *Barberrella* except three lines in a brief key. In a letter received from C. S. Brimley, dated September 8, 1926, he wrote: "Dr. Knight has been here and examined the type specimen of your *Pilophorus brimleyi*. He said it belonged to the genus *Barberrella* and was apparently new." Knight in his 8-page diatribe reverses this opinion and makes *brimleyi* a synonym of his *apicalis*. Until the types of the two specific names can be compared, I prefer to call the one from North Carolina *Barberrella brimleyi* (Blatch.). This statement may perhaps allay to some extent the "shakiness of the confidence" in the new forms described by me.

*Pilophorus cinnamopterus* (Kirschbaum). My basis for the inclusion of this species was the record of Osborn, accepted by Van Duzee. As Knight had not then expressed his opinion that it does not occur in this country, it was included on the records cited.

*Pilophorus amænus* Uhler. Dr. Drake has informed me that the error in the naming of my Fig. 179, as taken from his 1923 work, was due to Knight's having erroneously determined for him

the species taken at Cranberry Lake as *P. amœnus*. No mention of this error was on record at the time my text was prepared.

*Dicyphus notatus* Parshley. On October 19, 1925, after studying the original descriptions of both species, I wrote to Dr. H. M. Parshley, expressing the opinion that his *D. notatus* was a synonym of *D. vestitus* Uhler, and asked him if he thought otherwise to kindly point out any characters separating the two which I could use in a key. He replied rather abstrusely: "Knight has described a new *Dicyphus* from the east, and until I have had some comparisons made I cannot answer your question. *D. notatus* may be indeed the true *vestitus*—Shall write again later," which he never did. I had been unable to borrow specimens from Parshley and, therefore, could not study his *notatus*. Not wishing to make a synonym of it on the basis of the description alone, I included it as valid.

*Dicyphus vestitus* Uhler. In May, 1926, I sent in exchange specimens of what I had determined as *D. gracilentus* Parshley to E. P. Van Duzee, one of the oldest and best-known authorities on American Heteroptera. Under date of June 10, 1926, he wrote: "I wish to call your attention to the fact that the species you sent as *Dicyphus gracilentus* Parsh. is almost certainly that described by Uhler as *D. vestitus*. I have specimens of *vestitus* sent me by Gillette and Baker at the time their Bulletin came out which differ in no way from the specimens you send. I believe there can be no question as to this synonymy." Relying on this opinion of Van Duzee, which he afterward reiterated, I made *gracilentus* a synonym of *vestitus*, stating in the notes that the synonymy was on the authority of Van Duzee.

*Psallus variabilis* Fallén. This species I included on the records of Van Duzee the only ones extant at the time my book was published. Knight now states those records were based on examples of one of his recently described varieties of *Lepidopsallus rubidus*. This is, therefore, another of the twelve species of which Knight thinks I was greatly in error for including in my book.

At the close of his article Knight complains that I did not give him due credit for collecting specimens which I borrowed from other parties. The labels on these specimens did not have his name as collector. How was I to know who collected them? As stated in the footnote, page 6 of my work, he refused absolutely to lend me any specimens when he had thousands of them at his command. Had he done so I would have given him credit as I

did all others who favored me. He finally, for some unknown reason, relented and sent me, unrequested, a single Cuban specimen of *Sthenarus plebejus* Reut., for which I gave him due credit on page 923 of my work.

Dr. C. H. Drake, of Ames, Iowa, has recently gone over my collection of Tingitidae, with the intention of preparing a review of that family as presented in my book. He has since written me, "Your new species of *Melanorhopala* is probably a synonym." Dr. Drake has, of course, as much of a right to his opinion, as I have to mine, and I still consider it a valid species.

He also states, "I do not agree with your new Tribes of Tingitidae. If you take the genera under the tribes and consider the species of North America, or North and South America, or the world, you will have considerable difficulty in using your tribes." If he or any other critics of these tribes and other subdivisions used in my classification will turn to page 5 they will find this statement: "The characters used and statements made, both in keys and descriptions, are, for the most part, to be considered as applying only to those species occurring in the territory covered by this work. They may be, and doubtless are, capable of much wider application, but it is not safe to assume that such is the case." Again, on the top of page 450, there is also this sentence: "For convenience of treatment the subfamily (Tinginae) is separated into three tribes." My manual was designed principally to enable the students of Heteroptera in eastern North America to identify specimens from that territory. My tribes of Tinginae embrace and cover the genera of that area. Since a tribe or a genus does not exist in nature but is only an artificial concept of man to enable him the more readily to group his species, Dr. Drake, or any other person, is at perfect liberty to make new tribes of Tinginae or to amplify those which I have used, in order to cover the species of the world.

In a recent editorial in *Entomological News*,<sup>8</sup> Dr. L. O. Howard gave it as his opinion: "Scientific men, especially the entomologists, are growing broader and more unselfish—more considerate of one another, and more interested in the welfare of humanity as a whole." Judging from my personal experiences, it is evident that when Dr. Howard wrote those words he had not come in contact with some of the present day "quit-claim" specialists of this country.

<sup>8</sup> Vol. XXXVII, 1926, 300.